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DIVERSE IDEALS AND DIVERGENT CONCLUSIONS IN THE STUDY OF BEHAVIOR IN LOWER ORGANISMS[1]

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The living interest of the study of the behavior of animals lies in the concrete facts: in what the animals *do*. A kinematograph would perhaps make the best possible presentation of this subject. But to-day I am going to deny myself the pleasure of presenting the concrete facts, and deal rather, since this is a conference on research, with certain problems of investigation, in the behavior of the lowest organisms. I wish to try to present the main general results of investigation in this study of behavior at its lowest terms, together with its relation to certain general theories; to show why there is such marked disagreement in the accounts given by different investigators; and to point out the fundamental problems for future work. I realize only too strongly that I am not a psychologist, and that my claim to a hearing before psychologists lies only in that my work has been with matters which the psychologist needs to take into consideration. I shall therefore not deal with matters that are psychological in the subjective sense, but with some of the biological relations that underlie psychology.

In recent years a new spirit, a new desire, has permeated biological science in every division,—in brief, the desire *to see the processes of nature occurring*, and to modify and control these processes—not merely to judge what processes *must*

[1] Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

have occurred.[1] In the words of the young Clerk Maxwell[2] we wish to see the "particular go" of the processes of nature. This is the essential point in the present wide use of the experimental method in biology.

Contrasted with this is an earlier method of work, which may be expressed as follows: Certain conditions were seen to exist. From this, conclusions were drawn as to what *must have occurred*, in order that these conditions might exist. If we succeeded in imagining a process that would satisfactorily account for what exists,—then that was a sufficient explanation. If we found further that this explanation fitted certain other facts, which it was not devised to explain, then it was felt that the explanation was confirmed; was *verified*, even though the supposed process had not itself been observed.

This method of interpretation was long the common one throughout the general biological sciences, zoölogy and botany, and was much in use even in physiology and psychology. The great example is the theory of evolution, together with the special theories grouped about it, concerning the origin of particular organisms, or of particular structures and functions.

In the new spirit of work, the desire is to see things happening, not to conclude what must have happened. If evolution occurs, we wish to *see* it occurring; if acquired characters are inherited, we want to see a few acquired and inherited. We wish to see the processes themselves, not merely the result of supposed processes.

This is the spirit that has led to the recent rebellions against accepted doctrines. Its more thorough-going partisans reject all explanations that are merely devised to explain results. If they can't see evolution occurring, they conclude that it does not occur. They refuse to accept the principle of selection because they do not see it at work. They tell us, with von Uexküll,[3] that Darwinism is to be stricken from the list of scientific theories; with Driesch,[4] that Darwinism fails all along the line. Any explanation that deals with processes not observed is ruled out. The ideal is to build up an account of nature that shall consist entirely of statements that can be verified; that is, in which any process said to occur can be *observed* to occur, if the conditions are properly supplied.

[1] Compare the statement of the object of scientific investigation by Loeb, to whom perhaps more than to any other person the prevalence of this ideal is due. (Loeb, J., Preface to *Studies in General Physiology*, 1905.)

[2] As quoted by James, *Pragmatism*, p. 197.

[3] Die neuen Fragen in der experimentellen Biologie. Rivista di Scienza "Scientia," 1908, vol. 4.

[4] Science and Philosophy of the Organism, vol. 1, p. 269.

It is only by grasping thoroughly this ideal that one can understand the basis for the views set forth by its exponents; the startling conclusions drawn by such men as Driesch, v. Uexküll and Loeb.

The same spirit and method have been carried into comparative psychology, and the first result was to banish that which had heen supposed to constitute the very subject under examination. "Comparative psychology" became "animal behavior," for psychological processes cannot be observed in animals, and are therefore ruled out. Their very existence was called in question, and all discussion of them was denounced as idle speculation; comparative psychology was denied a place in science.[1]

This spirit has doubtless led to iconoclastic excesses in its zealous partisans; there has been a tendency to forget that within the next ten thousand years many processes may be observed that we have not succeeded in finding in the decade or two in which this spirit has ruled, so that sweeping negative conclusions are hardly warranted. But as a basis for a positive working method the ideal is sound and valuable. We must observe the processes of nature, not merely guess at them. Certain results and problems arising from the study, in this spirit, of the behavior of lower organisms, is what I wish to bring before you. Let us accept the tests imposed by this ideal, and see what we have reached.

Strangely, perhaps, this spirit of work has not succeeded in bringing about agreement in the study of behavior in the lower organisms. As you know, extreme divergences on what seem fundamental matters are found between the accounts given by different sets of experimental investigators in this field. Certain investigators show the phenomena as simple and dominated by easily understood mechanical factors; concrete physico-chemical explanations are presented as throughout satisfactory. The whole matter is set forth in such a way that he who runs may read and understand. Here, one feels, is a field that has been set in order; here the physico-chemical methods of attack have shown themselves adequate. And what a contrast to the results of older methods of study! No psychological discussions, no anthropomorphism, no teleology; no uncertainty, but simplicity, uniformity, constancy everywhere; simple mechanical considerations suffice for all. Truly a triumph of the mechanistic views of life phenomena!

But to the student's perplexity, he finds another group of

[1] See for example, v. Uexküll, *Im Kampf um die Tierseele*, reprinted from Asher and Spiro's Ergebnisse der Physiologie, Jhrg. 1, 1902, and Nuel, *La Psychologie Comparèe est-elle Légitime?* Archives de Psychologie, T. 5, 1906, pp. 326-343.

experimentalists, working with equal thoroughness, that give a very different account of these matters. They tell us that behavior in the lower organisms is extremely complex, varied and variable. The simple mechanical explanations are largely rejected as unverifiable and inadequate to the facts. The regulatory character of behavior, which has given rise to teleological and vitalistic doctrines, is insisted upon. Resemblances are pointed out between the behavior of lower organisms, and that of higher animals and man. The striking differences between the behavior of even the lowest organisms, and that of inorganic bodies is set forth.

The members of the first group of investigators thereupon accuse those of the second group of most grievous sins. They accuse them of indeterminism, of anthropomorphism, of "psychologizing," of teleology or finalism; of vitalism.[1] The members of the second group plead not guilty to these charges, and are inclined to respond by characterizing the work of the first group as superficial, misleading and generally inadequate.

Why this divergence? Why should investigators working on the same field under the same principles fail in this radical way to come to agreement?

The divergence appears to me largely due to certain differences in the plan and method of investigation; to difference in opinion as to what knowledge concerning behavior is of worth. There are two diverse ways of attacking the problems of behavior in lower organisms, and these two ways lead naturally to the two divergent sets of views that we have sketched.

1. The first method, and the one giving the brilliantly simple results, may be called the method of the physico-chemical key, or from certain points of view the *synthetic* method. It consists most typically in taking some single physico-chemical principle, or the action of some simple physical agent, and using this as a key to unlock the secrets of behavior. One takes osmotic pressure, or surface tension, or the electric properties of ions, and traces its operation throughout behavior; or he studies the direct action of gravitation, of heat, of light, on the movements of animals. That is, one keeps his eyes on this physico-chemical principle or agent; this, not the organism, forms the unit of work, the object of investigation. Experimentation consists in subjecting organisms to the operation of the principle or agent in question. When we have thus traced the action of the various physico-chemical principles or agents with which we are acquainted, we have the science of behavior; this science is thus built up synthetically from the simplest elements.

We have all seen this method applied, with brilliant results,

[1] For a compendium of such accusations, the recent book of Dr. Bohn may be consulted (*La Naissance de l'Intelligence*, 1909). See also Loeb, *Journ. Exper. Zool.*, vol. 4, 1907, pp. 151-156.

in many fields of biology. It has given us in behavior, among other things, the famous tropisms,—the direct, uniform effect of constantly acting physical agents on living organisms.

Those who work in this way commonly hold that it is the only method of work that is worth while. The purpose of investigation is held to be, to work out the physics and chemistry of biological phenomena. The way to do this is evidently to take our known physical and chemical laws, and trace their operation in the biological field. A fundamental principle for this method of work is this: physico-chemical action is constant; it can be depended on. With the same reagent acting on the same material, we must get always the same results. The organism is the material; the reagents to be used are the known chemical and physical ones; the results form our science of behavior.

This is the method of investigation which gives the plain and simple results. The ogres of anthropomorphism, of teleology, of vitalism, that have devoured so many biologists, nowhere so much as show their heads to the traveller upon this route.

2. The second method of study may perhaps be called the analytical method. It is based upon interest in the organism rather than on interest in physics and chemistry, and it makes the organism the unit of work, the object of investigation. The investigator wishes to know all about a given organism. Among other things, he wishes to know its entire behavior, and incidentally whether its behavior is as a matter of fact fully accounted for by known physico-chemical principles, and if not, how much and what is left over.

The actual method of work is to first watch the organism under its natural environment, till one finds out all things it does. Then the environment is changed a little, to see what difference this makes in the behavior. We thus try all sorts of different ways of getting the animal to change its behavior,—including the application of definite chemical and physical reagents of most varied character. We find all the different things the animal can do, and we work out the determining causes and conditions for each. We find all the different ways in which the animal may react to the same stimulus, and we find all the different methods of causing it to give each reaction. We thus find the organism's system of behavior and the things that influence it,—becoming acquainted with the creature as we might get acquainted with a person with whom we are thrown much in contact.[1] We carry on the same sort of work with different organisms, and compare them.

[1] Compare v. Uexküll's statement that the first requirement is "Die andauernde und eingehende Beobachtung des lebenden Tieres in seinem Milieu" (*Leitfaden in das Studium der Experimentellen Biologie der Wassertiere*, Wiesbaden, 1905, p. 75.)

Thus in this method we begin with the complex organism and attempt to analyze its behavior, proceeding to simpler and simpler determining factors, till we get the simplest that can be reached. In this way we must of course finally reach the simple physical and chemical factors with which the investigators by the other method start, provided that the latter are real factors. But since the analysis is difficult and slow, for a long time we are forced to deal with complex components; we separate the original complex mass into smaller and smaller complexes, each to be thoroughly analyzed later. These complex components are not elementary physical or chemical factors. We deal with organisms as wholes, and with such concepts as respiration; the securing of food; protection and defense; care of the young, construction of nests and the like; things which cannot now be expressed in the terminology of physics and chemistry.

Thus the investigator by this analytical method does not agree that work with the elementary chemical and physical factors is the only thing worth while; he finds, in contrast to the worker by the synthetic method, that the relations between these complex, biological components are in themselves decidedly worth while; that they are indeed of the greatest interest and importance. Such are the relations of organisms to each other; the preying of one on another; the conjugation of organisms; the selection of food, and other relations of behavior to metabolism; the relation of behavior to defence and protection, the similarities and differences between the behavior of different organisms, and the like. These things are not physico-chemical concepts, and the relations between them fall outside the field of view of the investigator who, in the synthetic method, works only with such concepts.

These biological interrelations form then a large part of the field of study by the analytical method. But it is of course clear that there is nothing incompatible in all this with further analysis into elementary physical and chemical factors. We merely find it impracticable and useless to wait until such analysis is complete before dealing with the important biological interrelations. Indeed, we should have to deal equally with these biological relations even if the analysis into physico-chemical factors were completed; they would be quite lost sight of if we limited ourselves to an account of the simple chemical and physical factors. The interrelations of mountain, stream and forest are important, but they are not easily set forth in terms of the movements of electrons or ions; and the working of a Hoe printing press is an equally refractory subject for presentation in that way. As Ritter has well emphasized in a recent paper "you can never give a full account of any whole in terms of its elements." [1]

[1] Ritter, W. E.: *Life from the Biologist's Standpoint.* Pop. Sci. Monthly, *75*, p. 177.

But of course the work of analysis is not finished, in the case of the organism, as in the landscape or the machine, until each part is resolved into the elementary determining factors with which the synthetic method starts.

Thus the synthetic method starts at the bottom and attempts to see how the complex behavior is built up from the simplest elements, while the other method starts with the given complex,—the organism and its behavior,—and attempts to resolve this into its elements. It is of course never possible to classify men absolutely, but typical examples of the different sorts of investigators can be given. For the synthetic method, beginning with the simple chemical and physical principles, the great example is of course furnished by Loeb and his followers. For an example of the investigators on lower animals that begin with the complex and attempt to analyze it, we may name von Uexküll. We may cite also the brilliant work done on behavior in lower organisms under Professor Hodge's direction, in this university. The present speaker has likewise followed this analytical method; and it has almost of necessity been the method employed by workers on higher animals.

Now, unless we are mistaken in our fundamental premise that physico-chemical explanations can be given for behavior, both these methods of work are quite justified, and they must finally, when they have finished their work and covered the ground thoroughly, come to the same results. But one method begins at one end, the other at the other end, and since "science moves but slowly, slowly," we find that at any given time the two have not met; they have not covered the same ground; they do not see the field alike,—and there arise misunderstandings and controversies.

The first method of exploring behavior may be compared to the method of the prospector who goes into a new country seeking gold. His eyes are all for the signs of gold; he may find it and come back laden, as Loeb has done again and again. But he is not so likely to bring with him a correct map of the country as is the surveyor.

The second method of exploring behavior may be likened to that of the geological survey. The first business is to make a topographical map of the country, so that we may guide ourselves and get a general view. The next is to make a systematic and detailed examination of all regions, to find out the relation of river and mountain and forest; of formations, strata and soils.

The surveying party is not so likely as the prospector to return from a given trip with gold, but it is naturally more likely to have made an accurate survey of the country, and to

have determined the various things that are there to be found. It may even be able to correct certain distorted views reported by the prospector. The investigator who works in this way is likely to give a more adequate 'natural history' of behavior than the worker by the other method. He will have a more complete idea of the matters remaining to be worked out, the difficulties to be met, and the relation of the different parts to each other.

It is the difference in the pictures presented by the men working in these two diverse ways that is the main cause of the controversies that have arisen. In other fields of biological investigation we find the same two methods of work, and there appears to be an almost "irrepressible conflict" between their representatives, though the divergences in results are perhaps less crying than in behavior. My present purpose is to try to compare in certain general features the picture presented by the two different sets of workers, in the behavior of lower organisms, pointing out the ground for the divergences.

In the unicellular organisms we have theoretically the simplest condition of affairs that we can find; we have the problem of behavior at its lowest terms. What are the characteristics of this behavior? What bearing have the phenomena here on the possibility of giving physico-chemical explanations for natural phenomena? What relation have they to those three reprobated tendencies or doctrines of which we hear so much of late, in attack and defence, in accusation and recrimination: to vitalism, to anthropomorphism, to teleology or finalism? Why do these doctrines maintain themselves, and how far is there justification for them?

Now, it is evident that the first or synthetic method of study, from its very nature, avoids any phenomena on which these reprobated doctrines could possibly be based, any phenomena not manifestly of a physico-chemical character. It simply does not look at them. It devotes itself to visible physical and chemical matters, leaving the rest out of the field of view. This is done deliberately and intentionally, because it does not consider other matters ripe for treatment. The point of view of such investigators is well expressed by Bohn, in his recent eulogy of Loeb: "as a man of positive science, he does not waste his time reasoning about things that have resisted scientific analysis."[1] I believe that no criticism can be made of this procedure, as a method of work, provided it does not represent as non-existent the parts with which it does not deal. Its chief danger is that it tends to make one forget how much remains

[1] "—En homme positif, il ne perd pas son temps à raisonner sur des choses qui ont résisté à l'analyse scientifique." Bohn, *La Naissance de l'Intelligence*, 1909, p. 44.

to be done. To point out the limits of our present knowledge and the problems that remain to be solved is almost as important as to set forth what has already been worked out. And this is particularly true in fields where that which is understood is but a fraction of that which exists. The investigator indeed must keep his eye on what remains to be done, rather than on what has been done.

Thus the freedom from anything vague or uncertain; complex or difficult to understand; anything not resembling common physico-chemical action; anything that could suggest anthropomorphism or finalism or vitalism,—that we find so striking in the accounts of investigators working by the synthetic method, is after all *a priori*, and due simply to the omission of everything of that sort. The reader, finding in these accounts nothing to suggest the difficulties, concludes that the difficulties do not exist; that this is a field where the resolution into physics and chemistry has become complete.

But the worker by the analytic method is unable to escape so easily from the difficulties. Making the complex organism his object of study, interested in this for its own sake, and attempting to omit nothing, he finds himself at once confronted with a mass of phenomena that have not been analyzed into physico-chemical factors; phenomena in the highest degree complex, varied and peculiar. He is even pained to find that many of the proposed physico-chemical explanations are inadequate for the very phenomena which they have been called in to explain. He is forced to deal with that storehouse of materials from which anthropomorphism, finalism and vitalism have drawn their supplies. He endeavors in a preliminary way to analyze and arrange these, and to report to those interested, what one finds when one examines thoroughly the behavior of lower organisms. If he is honest, he is compelled to report even in the lowest organisms many features that resemble features in the behavior of man. He finds inescapable relations between the present actions of the organism and certain later conditions,—resembling what is seen in the purposive action of man. Though these discoveries may be unwelcome, the surveyor with a conscience is forced to set them down on his map and mention them in his description.

And then the storm bursts upon his head. Anthropomorphism! Teleology! Finalism! Vitalism! cries in horror the aggressive physico-chemist, and the rash investigator is drummed out of the mechanistic camp. The teleologist and the theologian seize upon the account of actions resembling those we perform with a purpose, and count the author among their allies. The upholders of vitalism hold out the right hand of fellowship to

the investigator who has shown the inadequacy of physico-chemical explanations.[1]

Yet the unfortunate investigator whose sin has been to try to tell the whole truth is still a believer in the validity of physico-chemical explanations; in the necessity of formulating all processess causally; he holds that teleology cannot be substituted for causation, that to perceive the resemblance between man and lower animals does not give a causal explanation of either; he is convinced of the error of vitalism. Rejected from the camp of the physico-chemists, and refusing to take refuge with the enemy, he is left an outcast.

Where is the mistake? Do such accounts of behavior as he gives really imply that physico-chemical explanations are wrong in principle,—as both the vitalists and the aggressive physico-chemist seem to agree that they do?

If they do—alas for physico-chemical explanations—for such accounts are certainly correct! But how incredibly short-sighted; how faint-hearted, is the physico-chemist that takes such a view. These accounts of behavior neither justify finalism and vitalism as substitutes for causal explanations, nor do they refute mechanism. They show merely that there is still much work for us to do; that the end of analysis is by no means in sight; that some of the methods of analysis have been inadequate; that the problems are much more extensive and difficult than some have imagined. They have no more weight in overthrowing physico-chemical explanations than has the existence of an unexplored country, in showing the impossibility of exploration. True, the choice between physico-chemical and vitalistic formulations must for the present, for a large part of the phenomena at least, be based upon *a priori* grounds, not upon demonstration. But whoso has imagined otherwise should remember the saying of Newton, that he had but picked up a few pebbles on the shore of the ocean of knowledge, and should ask himself whether he has really gathered into his pocket that measureless ocean and all its shores. When he has done that, he may have established by demonstration the complete adequacy of physico-chemical explanations; until he has done it, we must rely upon *a priori* considerations.

If we accept from beforehand the programme of physico-chemical or mechanistic explanation, how are we to conceive the facts in the behavior of lower organisms? What are the chief facts discovered in a careful survey; what is their relation to vitalism, to anthropomorphism, to teleology?

[1] See for example Driesch, *The Science and Philosophy of the Organism*, vol. 2, and v. Uexküll, *Die neuen Fragen in der experimentellen Biologie*, *Rivista di Scienza* "*Scientia*," vol. 4, 1908.

1. Let us consider vitalism first. The experimental investigator is interested in vitalism on account of its practical bearing on the justifiability of his methods of work. He can experiment only by altering in some way the configuration of matter and energy: *i. e.*, by making some physical or chemical change. Can he hope to carry through this method consistently and completely, explaining all divergences in results by differences in the preceding configuration of matter and energy? Or will he find cases of divergence in results where there are no foregoing differences in the configuration of matter and energy, so that from throughout identical configurations diverse results follow? If the latter is the case, then evidently the experimental method fails.

The question here is not merely whether the explanations hitherto commonly used in physics and chemistry are entirely adequate ones. It is the deeper question, whether when adequate principles of explanation *are* used in the physics and chemistry of things not alive, other and diverse principles of explanation will be required for the study of living things. The question is as to whether there is a real, fundamental diversity between the necessary principles of explanation for living and for non-living things.

To hold that the necessary principles of explanation are not diverse in the two cases; to hold then, in opposition to vitalism, that physico-chemical principles of explanation are generally adequate, is to hold this:

What happens in any system of matter and energy at any period is determined by the configuration of matter and energy at a preceding period. Experimentally, therefore, differences in resulting conditions in any two cases will always be found preceded by differences in foregoing conditions, so that nothing happens without its determining factors in the previous configuration of matter and energy. If we search with sufficient care, we shall always be able to find in matter and energy a determining factor for everything that occurs. Two identical combinations of matter and energy cannot produce different results, nor two different combinations absolutely identical results.

This of course leaves open the question whether there may not be present in certain cases additional, subjective, properties, and also, whether it may not be proper to make, besides the physico-chemical explanations, other explanations that take into consideration these subjective properties. Furthermore, it does not deny that there are in living things combinations of matter and energy not found elsewhere, so that methods of action may occur that are not found elsewhere; to this we return later.

In attempting to understand and explain the behavior of

the lower organisms, one of the chief stumbling blocks has been a preconceived idea as to what we should find. Many have imagined these creatures to be a sort of link between organic and inorganic material. Higher animals it is generally recognized are complex structures, comparable in this respect to machines. In the lower organisms it was expected that we should find mere masses of a certain sort of material, and the study of behavior would be merely the study of the chemistry and physics of this material. This idea has perhaps been most explicitly developed by Le Dantec.[1] This *a priori* conviction, often unformulated, maintains itself most obstinately in all thought on the behavior of lower organisms, particularly of those composed of but a single cell.

The most fundamental result of the study that has been made is to show the incorrectness of this idea; to show that lower organisms, like higher ones, are typical *arrangements* of material; are structures; are in this respect machine-like; not masses of a uniform substance.

This is evident both from a study of the activities of these organisms, and from a direct examination of their structure. Such a unicellular creature as Stentor has many systems of differentiated organs, often acting independently of one another; its structure can only be characterized as inconceivably complex. Paramecium with the microscope is seen to be a most complicated machine, running at a high rate of speed. In studying the behavior of these creatures, what we find out is *how certain machines work*, rather than the direct physical and chemical properties of a certain substance.[2]

The practical difference in the results of study, due to this fact, is perhaps much greater than appears at first thought. To this difference between what was expected and what was found are due most of the paradoxes and difficulties in behavior, and most of the apparent failures of direct physico-chemical explanations. To it are due the recrudescence of anthropomorphism, finalism, vitalism. Let us try to see how these things come about. In so doing I shall have to remind you of certain facts that are sufficiently obvious, yet that have been neglected of late in work on lower organisms.

The first principle of physico-chemical explanations is that the chemical and physical properties of the living substance

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[1] *La Matière Vivante*. Paris, 1895.

[2] Whether, from other points of view, an organism is not also something more than a machine, is another question. If we mean by a machine any system in which the configuration of matter and energy determines what happens, then the contention in the text is that for purposes of causal explanation of what happens only this machine-like character requires consideration.

determine its reactions, so that from a knowledge of these properties we can predict the reactions; and further that the same reagent acting on the same substance is bound to produce the same result. Any statements to the contrary are looked upon as controverting the principle of causal determination and leading to vitalism.

But when the substance is arranged in typical ways, this principle, though essentially true, becomes practically false and misleading. From the same mass of substance we can make many different arrangements or machines, acting in entirely different ways, so that we could never predict the reactions of the machines from a knowledge of the chemical and physical properties of the unarranged substance. From a certain mass of material we could make either a clock or a doorbell or a steel trap or a musical instrument,—and we could easily so arrange these that each would respond in its characteristic way when acted upon by an electric current. We could, moreover, make the same machines, showing the same reactions, from a different kind of material, with different properties. We could then never predict the reactions of these by knowing merely the chemical and physical properties of the material of which they are composed. The specific action of each depends on the specific arrangement of its material.

This is exactly what we find in organisms, including the lowest as well as the highest. From it there result certain relations that are extremely perplexing, though they can be illustrated from inorganic combinations as well as from organic ones. Let us examine some of these.

First, both in inorganic arrangements or machines and in organisms we find that the same substance reacts to a given reagent sometimes in one way, sometimes in another. It all depends on how the material is arranged;—whether as a clock or as a doorbell; whether as a Stentor or as a Paramecium. Furthermore, by a slight shifting of the arrangement, we find that the very same piece of material is caused to react to the same reagent in entirely different ways. A typewriter responds to mechanical stimulations by printing English lower case letters. After momentary pressure of a certain lever, it responds to the same stimulations by printing capital letters, or by printing numbers or conventional signs. It would be easy to so arrange it that after a shift of a lever, it would respond with script or italics or German or Russian or Greek letters. Here we have parallel conditions to what we find in lower organisms. What we find to be true for one organism or one individual turns out not to be true for another, and even what we find to be true for a given individual will not hold later for the same

individual. The principle of the *shift* in arrangement comes into play continually, giving us most inconstant results. Yet there is no breach of determinism here; the determining feature lies both for the machine and for the organism in the arrangement of parts.

In view of these facts, it is not surprising that we often get nothing fundamental by determining the action of a given chemical or physical agent on living substance. It is often assumed, tacitly or openly, that after such a determination is made its results can be transferred to other masses of living material; can be generalized. But the effects seen are usually due to the characteristic arrangement of the material acted upon; they disappear or are reversed as soon as we work with material in other arrangements. We cannot therefore expect the study of the direct simple action of chemical and physical agents to help us greatly in understanding why an animal does what it does, though this has been heralded as the one right way to study behavior. From the action of the same agent most diverse results follow, depending on the arrangement of the material on which it acts.

Second, we find, both in machines and in organisms, that a mass of substance may respond in the same way to reagents of the most diverse kind, reagents having varied and even opposite effects. The avoiding reaction in Paramecium may be caused by heat and by cold; by acids, by alkalies, and by salts; by electrolytes and by non-electrolytes; by increase of osmotic pressure and by decrease of osmotic pressure; by chemical action without change of osmotic pressure; by mechanical shock and by electric shock; and this is a type of what we find in organisms. How often of late in the history of comparative physiology have we seen a certain effect attributed to one specific physico-chemical principle after another,—always on the assumption that the direct physical action of the agent is the essential point. Increase in osmotic pressure is first heralded as the essential point, until we find that decrease in osmotic pressure has the same effect, and that chemical change without change in osmotic pressure has likewise this effect. Then positively charged ions are made responsible, till we find that negatively charged ions will produce the same effect. And so the gamut is run; till heat, cold, mechanical shock, and the most diverse agents are found acting in the same way.[1]

Parallel results are easily obtained from combinations of inorganic material. Imagine an arrangement such that when an electric connection is made, some characteristic action is per-

[1]For an example lying outside the field of behavior, the history of the theories of artificial parthenogenesis is most instructive.

formed; for example, a bell is rung. It is easy to so arrange that reagents of opposite character shall make the connection and so ring the bell. Thus the electric button could be placed on a support between and close to an upper and a lower post, contact with either of which completes the circuit. Now, heating the support would lengthen it and cause contact with the upper post, while cold would shorten it and cause contact with the lower post; in either case the bell would ring. Similarly, it would be easy to so arrange a circuit that addition of acid or alkali or any electrolyte would close it; or even so that non-electrolytes would have the same effect. By having a somewhat complex structure, the most varied reagents would all close the circuit and ring the bell. This is the sort of thing we find in organisms. From diverse reagents we may get the same reaction; from the same reagent we get sometimes one result, sometimes another.

It is this condition of affairs that has brought upon the head of the investigators that report it accusations from the narrower physico-chemists of indeterminism, of vitalism, and from the vitalists claims that physico-chemical explanations have failed.

Yet we find the same relations in the inorganic combinations that we call machines. In machines do the physico-chemical properties of the material determine its action? Is it proper to study these physical and chemical properties in order to understand how the machine acts?

Evidently these questions are to be answered *yes!* Only, we must study these properties intelligently, recognizing the fact that the properties due to a certain arrangement of material are among the most important of all, for it is to this arrangement that the specific action of the machine or the organism is due. From a mere study of the properties of unarranged iron, ivory, paint, ink, and of the action of various agents upon them, we could never understand the typewriter. So from a mere study of the unarranged material of organisms and the action of agents upon them, we can never hope to understand their behavior.

Herein lies the failure or inadequacy of much of the physico-chemical work along these lines; it has dealt mainly with the properties of unarranged material, when the arrangement is precisely the essential point. There is among certain general physiologists an intense prejudice against all morphology; a cardinal point of faith is that structure is of no account. By taking this stand, they play directly into the hands of the vitalists. Vitalistic theories flourish as a result of too simple statements of the problems, and too simple solutions, on the part of the physico-chemists. If we maintain that physico-chemical explanation means that behavior in

lower organisms is the direct result of simple chemical and physical action on a certain kind of substance,—then such explanation undoubtedly fails, and the vitalist triumphs.

What is most needed is that the physico-chemical student of biology shall realize that as matter takes on new arrangements, its activities and reactions become different, even though the properties of each constituent part may remain the same. Since in living things there are beyond doubt arrangements differing from anything found elsewhere, we are of course certain to find in living things ways of acting that differ from anything found elsewhere. Hence we cannot expect to find in the physics and chemistry of inorganic matter the full explanation of the activities of organisms; those who expect to do this are following a will-o'-the-wisp,—and this is certain from physico-chemical principles themselves. The physicist and chemist must study organisms in order to fully understand physics and chemistry, just as the biologist must study physics and chemistry in order to understand organisms.[1]

The arrangement of the material is then the essential point in determining behavior. What then becomes from the mechanistic standpoint the fundamental problem of behavior? Is it not the question of how these arrangements of material arise, and how they become changed? This property of taking on typical arrangements and of changing these arrangements is the fundamental and essential property of living matter that requires study for the physico-chemical understanding of behavior.

Of such study hardly a beginning has been made. With regard to the minute internal physical changes in the taking on of new arrangements, we have merely some faint suggestions from the rearrangement of particles in the hysteresis of

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[1] Some writers have applied the name vitalism to the idea that new methods of action arise when new combinations occur, taken in connection with the view that new combinations are found in living things; so apparently Radl (Geschichte Biologischer Theorien, vol. I, p. 81). But such vitalism involves no new principle of explanation; it is based upon conditions found in chemistry and physics as decidedly as in biology. New methods of action arise when oxygen and hydrogen combine, producing water; new methods of action arise when a mass of brass and iron is arranged in the form of a clock. How then can it fail to be true in the case of organisms? The study of physics and chemistry is the study of the methods of action of matter and energy, whether simple or in combination. No new principle of explanation is involved simply because the combinations studied are those in living things. This view seems therefore to the writer far from a vitalistic one. On this point, see the excellent discussion by O. Hertwig, *Der Kampf um Kernfragen der Entwicklungs- und Vererbungslehre* (Jena, 1909), pp. 55-81; notably p. 80.

colloids. A little more has been done in tracing the external manifestations of these changes in the individual organism, through the study of changes of behavior in different physiological conditions, and in the formation of habits and associations. The most important part of the problem lies in the question of how diversities of arrangement arise during the racial history.

It is here that the problem of behavior opens into the more general problems of heredity, variation and evolution. This is the field that underlies the study of behavior. How does the living substance become modified in the lapse of time, so as to take on new arrangements, and therefore to behave differently? On this, if we stick to our ideal of demanding to observe the processes as they occur, little has been accomplished. Von Uexküll, with this ideal in view, feels that a correct statement is this: that all we know about this process is that it does not occur as Darwin thought it did.[1] This is the central physico-chemical problem for a causal understanding of behavior. Compared with it, the study in living things of the changes in surface tension, osmotic pressure, the nature of solution, and the like, though important in themselves, can give us little help, save as they may underlie and finally lead to a knowledge of the properties and laws in accordance with which living matter takes on and changes its method of arrangement. This central problem of behavior is likewise the central problem of all biology.

Thus the condition of affairs we have sketched does not lead to indeterminism; to belief in the failure of physico-chemical explanations; to vitalism. It merely leads us to perceive that the problem is more complex than has been supposed; that the fundamental question is that regarding the production of arrangements in the living substance, and that the solution of this problem lies in the future.[2]

We have spoken of vitalism. What we have thus far set forth bears upon the problems of anthropomorphism, and of finalism or teleology, almost as directly as it does upon vitalism. We need therefore to touch upon these but briefly.

2. Anthropomorphism signifies unjustifiably reading into lower organisms the characteristics, particularly the mental

[1] Von Uexküll: Studien über den Tonus, V. Zeitschr. f. Biol., vol. 50, p. 168.

[2] I need not say that this discussion is not presented as a "refutation" of any special brand of vitalism, for example, that of Driesch. For such a refutation the precise arguments set forth in support of vitalism would have to be taken up and overcome. I have here tried merely to set forth the attitude I have reached after some years of work on behavior.

ones, that we find in ourselves, and especially it signifies substituting these for a causal explanation. This is a serious error. But it has nothing to do with another question, with which it is often confused. This other question is, whether the behavior of animals resembles in any features the behavior of man. This is purely a question of objective fact, not one for prejudice or for *a priori* considerations of any sort. The only way to answer it is to learn the objective facts for man and for animals, and then to compare them, observing where there are resemblances, where differences. To see such an objective comparison proposed seems to arouse all the fighting instincts of some of our ultra physico-chemical friends. just as it does some of our theological friends. But if we persist in making it undisturbed, we undoubtedly find many fundamental resemblances, along with many differences, between the behavior of even the lowest organisms and of man. Some of these are: the fact that reactions are due to the release of internal energy; that action may occur without specific external stimulus; that action is modified by internal changes of the most varied character, many of which are parallel in man and protozoan; that negative reactions are given mainly to injurious agents, positive ones to beneficial agents; that varied reactions occur under the influence of a single constant stimulus, and that the organism tends to persist in that reaction which keeps it in conditions favorable to its life processes.

To point out these and similar resemblances is merely to point out the facts, with which we must all come to some sort of a working compromise. The existence of organisms whose behavior has these characteristics is not inconsistent with physico-chemical explanation, if physico-chemical explanation is valid, for man does exist. No question of fundamental principle is then involved when we find that other organisms have these characteristics; the question is merely as to the distribution of characteristics known to exist.

The problem of behavior then is : How are such characteristics to be explained from the physico-chemical point of view? If the practical physico-chemist declares that they cannot ultimately be so explained, he merely condemns his own methods of work and places himself among the vitalists. For such faint-heartedness there is surely as yet no justification! We have indeed to attack certain problems hardly yet approached, —the study of the physico-chemical properties in virtue of which certain materials become arranged in such characteristic ways as to produce the phenomena we have mentioned.

3. The matter of teleology or finalism is in similar case. We find as a mere matter of fact, certain marked relations be-

tween a present process and something that exists later. These relations resemble in many ways the relations between action that in man is accompanied by a purpose, and the result of that action. We find such relations in many fields besides behavior; the physiologist is compelled to recognize them everywhere. In the study of behavior it has become a sort of popular fad to ignore these relations; to act as if they did not exist. These are merely the tactics of the ostrich. We must face the problems which nature sets us. When the lens of the eye develops in the dark, it is mere cowardice to try to act as if we did not know that this lens is so constituted as to bring light to a focus. When the pancreas secretes an enzyme, it is again struthionic tactics to refuse to see the relation of that enzyme to the digestion of food,—although this digestion does not take place till after the secretion has occurred. Behavior in lower organisms is an almost continuous tissue of such relations; they are largely what we call the regulatory features of behavior.

When the ultra physico-chemist assumes that the pointing out of these extraordinary relations is given as an explanation of them, he shows most surprising naïveté. They form precisely the most difficult, the most complex, problem for causal explanation. The question is; How were such relations brought about? This is a question for precisely the same kind of objective investigation as the question how any other relation was brought about. How does the boulder happen to lie in the middle of the plain? How does the lens get into the optic cup? How does the lens happen to be of such a form as to bring light to a focus? How does the animal happen to react in such a way as to protect itself? The answer in every case lies in tracing the processes by which the relations were brought about, and in discovering the laws of these processes; by beginning with the condition where these relations do not exist, and tracing the "particular go" of the changes until these relations do exist.

How the relations that impress us as teleological were brought about constitutes undoubtedly a set of most difficult problems. But to keep us from despairing, we find this process taking place in the lives of individuals in a manner that can readily be studied. This is in the formation of habits. In the formation of habits, we see that the organism at first does not react in a way that impresses us as teleological, while later it does, and we can watch the process of change from one condition to the other, and discover how it is causally determined. Since then a method of action that appears to us teleological is produced in an intelligible way under our very eyes, in the lifetime of the individual, there is no reason why we

may not expect to find out how teleological relations have been brought about in the life of the race, when we have actually made a start in the study of the physiology of racial processes.[1] It seems clear that the apparent relation of a present process or structure to something that comes later in time is always due to the fact that this future something has in fact acted upon the organism in the past. The present condition fits the future condition only because of a certain constancy in the universe, through which the 'something past' reappears again in the future.

Let us then attempt in closing to characterize behavior as we find it in its lowest stages, recapitulating the chief points we have made. We find in lower organisms, as in higher animals, that the nature of the reactions is due mainly to characteristic *arrangements* of material, not to the properties of simple unarranged substance. These lower organisms therefore furnish problems which do not differ in kind from what we find in higher animals. They are simpler only in a numerical sense, — in that their parts are less numerous than those of higher animals. It would be most interesting if we found in these lower creatures a half way stage to inorganic matter ; if we found living matter without characteristic arrangements, so that its properties and actions were those of an undifferentiated substance. But perhaps the chief result of research is to show that we do *not* find this condition realized. Doubtless this is a great disappointment, much diminishing the supposed importance of studying the lowest organisms. But we must bow to the facts.

In the behavior of these lower creatures we do not find that uniformity which certain physico-chemical theories of behavior demand. With certain underlying conditions in common, extreme diversities in methods of action are the rule. To the same stimuli different organisms react differently; different individuals of the same species react differently, and even the same individual reacts differently at different times. As Walter has well said "strictly speaking, all behavior is individual behavior,"[2] and Driesch[3] has shown his usual acumen in setting

[1] The statement is sometimes made that we can never hope to reduce teleological relations to causal relations (see v. Uexküll, Leitfaden in das Studium der experimentellen Biologie der Wassertiere, 1905, p. 129). This doubtless means that the teleological relations remain, even after the process by which they are brought about has been explained causally. It certainly cannot be maintained, in view of the known process of forming useful habits, that the process itself cannot be understood from a causal standpoint.

[2] Walter, H. E.: The Reactions of Planarians to Light. *Journ. Exper. Zoöl.*, *5*, 1907, p. 97.

[3] Driesch, H.: The Science and Philosophy of the Organism, 1908-9.

forth individuality as the central problem of biology. For the purposes of physico-chemical explanation, individuality in behavior is an outgrowth of the fact that there is practically infinite diversity in the arrangement of living substance, so that no two specimens of it are precisely alike, therefore they do not act precisely alike. Behavior in lower organisms presents itself exactly as it would if the theory of descent with unlimited modifiability were true, — every individual forming a centre from which modifications may and do diverge in many directions. If we abandon for a moment our requirement of seeing evolution occur before we accept it, and assume that it *has* occurred, then we should hold that lower organisms are not really more primitive than higher ones; each may have as long a history and as many modifications behind it as a higher animal. They would be conceived merely as creatures that have retained certain numerically simple arrangements, because they thus fit some otherwise unoccupied nooks and crannies of the universe.

The great problem of behavior then, as for biology in general, is to work out those properties of living matter and of the environment, by which characteristic arrangements of material are produced and modified. In all these typical arrangements or structures that we call organisms, including the protozoan and man, there are certain common features in behavior; the pointing out of these common features is at times denounced as anthropomorphism. Again, in the behavior of these typical arrangements of material, we observe certain relations of present actions to later conditions; these relations we call teleological. But how these conditions and relations are brought about is essentially a physico-chemical problem, in the sense that we can study only the processes and configurations of matter and energy from which they result. The entire actual situation in behavior is that on which theories of vitalism have been based. It can truly be said that the condition of affairs found in behavior is very nearly that set forth by the vitalists as a basis for vitalism; their conception of the gross facts of behavior is more nearly accurate and adequate, from a descriptive standpoint, than is that set forth by the simplifying physico-chemists. The vitalists take into consideration all the phenomena, while the ultra physico-chemists leave out of account the most interesting ones.

But this does not mean that vitalism is anything more than a name for what we have not yet worked out; it does not mean the giving up of the ideal of essentially physico-chemical or mechanistic explanation. It means merely that we must recognize the enormous complexity and difficulty of the physico-

chemical problem;[1] must realize that we have hardly gotten hold of the first threads for unravelling the puzzle yet; that we have, indeed, hardly attacked the real problem, save in the mass and by analysis into components that are themselves inconceivably complex. The central problem, of working out the laws and processes by which typical arrangements of matter and energy are produced and modified in organisms, presents itself as a problem to be attacked only by the essentially physico-chemical methods through which all the real causal explanation that we have has thus far been reached.

[1] "Nature cannot be made simple by treating her on the theory that she ought to be so, when as a matter of fact she is not." Ritter, l. c., p. 187.